SOME REMARKS

ON THE

VALUE AND NECESSITY

OF THE

NUMERICAL OR STATISTICAL METHOD OF INQUIRY AS APPLIED TO VARIOUS QUESTIONS

IN

OPERATIVE SURGERY.

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TO

DR HENRY MARSHALL,

DEPUTY-INSPECTOR OF HOSPITALS,

&c. &c.,

WHOSE STATISTICAL AND OTHER WRITINGS

HAVE CONFERRED

INVALUABLE BENEFITS ON MEDICINE AND MANKIND,

THE FOLLOWING REMARKS

ARE RESPECTFULLY INSCRIBED.

STATISTICAL PROPOSITIONS, &c.

" La possibilité de l'application de la statistique à la medecine, est une verité tout aussi bien demontrée que la realité de la circulation."—*Dezeimeris, Dictionnaire de Medecine,* vol. xxviii. p. 550.

THE vast practical importance of the doctrine of statistics, and its power of elucidating, simplifying, and deciding many and various inquiries in surgical and medical science, is now becoming daily more and more acknowledged by the members of the profession. The doctrine itself has been long, not only acknowledged, but acted upon by governments and by the public at large. The political laws and expensive machinery pertaining to the registration of the deaths and diseases of the inhabitants of England, and of other kingdoms of Europe, are founded upon the soundness of the doctrine. In our numerous life assurances and annuity companies, millions of money are unhesitatingly staked upon the truth of it. And the principle upon which the usefulness and stability of the whole doctrine of medical statistics rests, is a very simple one. It amounts to this: Among facts, data, or unities of a variable chance—such as the probabilities of death within a given time, or the probabilities of attacks of particular diseases within a given time, or the probabilities of averting death in particular diseases by particular methods of treatment, or operation—there is ever a mighty uncertainty as to the results, if we consider only single cases, or a small and limited number of instances; but our results approach more and more to certainty, in proportion as we deduce these results from a greater and more extended number of instances,—from a larger and multiplied series of

facts. There is always great uncertainty and instability in regard to the results of single or isolated cases; but a proper aggregation and conjunction of cases affords results which are comparatively certain and stable. For the great and leading principle upon which all statistical inquiry is grounded, consists in the fact, that in unities or entities of a doubtful chance, while the result, or event, in *individual* instances, is ever variable and uncertain, the result, or event, when calculated from, or upon *masses* of instances, becomes comparatively certain and invariable.

The doctrine itself of statistics, and more particularly the value of the statistical method of inquiry, as applied to the investigation and determination of various practical points in surgery, is still, I fear, very imperfectly understood by the profession generally in this country. It is on this account, and in consequence of my sincere conviction of the great importance of statistics for the future promotion and advancement of surgical science, that I here venture to offer a few very imperfect observations upon the subject. In following out this idea, I shall content myself with endeavouring to show the truth of the abstract remarks which I have above made, in the form of illustrations of a few of the fundamental principles or propositions upon which the doctrine of medical statistics is founded; and this more especially with a view to the bearings and important advantages of the statistical or numerical method of inquiry as applied to the study and settlement of different questions in the surgical department of the profession.

First Proposition.—*The absolute number of deaths from all causes, in a given time, in a given population, is always nearly the same.* The probability of life or death to individuals within the limits of a given period, is proverbially uncertain. Nothing is more uncertain, for instance, than the number of individuals that will die in the currency of a single year in any particular family, street, or village in England and Wales. But nothing could be more certain than, *cæteris paribus*, the number that will die during the currency of a single year in the whole of England and Wales. Estimating, for instance, as we do in *all* modes of reasoning and philosophizing, from the experience of the past what will be the experience of the future under similar circumstances, we may state beforehand as certain, that in 1845 (the results of which have not yet been published) the total number of deaths in England and Wales amounted to about 350,000. For the returns of the Registrar-General for England and Wales have now been collected and published for seven years,[1]—viz., from 1838 to 1844 inclusive, and the total numbers that died during each of these years were fixed and determinate, to the extent shown in the following table:—

[1] See the official "Annual Report of the Registrar-General of Births, Deaths, and Marriages in England" for the years named.

No. I.—*Table of Absolute number of Deaths in England and Wales, and of the per centage of deaths among the whole population during Seven successive years, from* 1838 *to* 1844 *inclusive.*

Year.	Absolute Number of Deaths.	Per centage of Deaths among the Population.
1838	342,547	2.2 in 100
1839	338,979	2.1 in 100
1840	359,634	2.2 in 100
1841	343,847	2.1 in 100
1842	349,519	2.1 in 100
1843	346,446	2.1 in 100
1844	356,950	2.1 in 100

In reference to the preceding table, we must bear in recollection one point, that in this, as in other statistical inquiries, there is always a range of oscillation, and limits of possible error; but, as Gavarret has well demonstrated, the extent of these oscillations and limits of possible error are themselves easily ascertainable, and capable of being reduced to mathematical calculation and correction.

Second Proposition.—*The absolute number of deaths from individual diseases and specific causes in a given time, in a given population, is always nearly the same.*—For if it be true, as shown under the first proposition, that the exact number dying annually in England is nearly the same, it is equally true and demonstrable that the particular causes or forms of disease producing these deaths recur in successive years in the same number and proportion. What is true regarding the whole, is true in regard to its parts. For the purpose of illustrating this secondary fact, I shall take from the Registrar-General's reports nine returns, three referring to medical, three to surgical, and three to obstetric affections and complications; and I shall add one pertaining to the department of medical jurisprudence. Each of them shows the comparative certainty of large numbers. For while, for example, no man could predict who or what number of a small community would die annually of croup, or tetanus, or ovarian dropsy, yet the absolute number dying each year of these and other affections throughout England, when calculated on a large scale, comes annually in all, except epidemic and zymotic diseases, to be nearly the same, as the following table sufficiently demonstrates. In fact, their numbers are, if possible, more determinate than the numbers of the total deaths; because, while the absolute mortality of a kingdom is liable to be varied by variations of a temporary and transient nature in the existing epidemic and endemic influences, &c., those individual diseases and causes of death, the etiology of which is more fixed, are more stable in their results.

No. II.—*Table of Absolute number of Deaths annually in England and Wales from twelve different Diseases or Causes of Death.*

Causes of Death.	No. Dying in 1838.	No. Dying in 1839.	No. Dying in 1840.	No. Dying in 1841.	No. Dying in 1842.
Croup, ...	4463	4192	4336	4177	4457
Jaundice, ...	841	800	875	864	952
Apoplexy, ...	5630	5293	5451	5581	5361
Hernia, ...	507	474	480	475	529
Tetanus, ...	129	122	142	118	118
Carbuncle, ...	35	38	33	28	40
Childbirth, . . .	2811	2915	2989	3007	2687
Malformations, . .	166	214	211	206	217
Ovarian Dropsy, . .	45	34	43	44	52
Violent Deaths, . .	11,727	11,632	11,594	11,100	11,092

The regularity with which the same disease thus destroys, in successive years, nearly the same number of individuals, may appear remarkable to those who have not given attention to the study of medical statistics, and who have, consequently, not marked the fixed and determinate nature of the results which this means of investigation always elicits, when it is enabled to work upon a sufficiently large basis of facts, or a sufficiently large series of data. But this constancy appears, if possible, still more singular, when we turn to such a subject as that included under the last column in the table, viz. "violent deaths." Under this head are included deaths by mechanical injuries, by chemical injuries, by asphyxia, &c., and, if the returns were more specific, it would no doubt be found that the number of violent deaths from *each* separate division of causes was annually nearly the same. Even causes originating in passions of the human mind, and leading to violent death by murder, are, *cæteris paribus*, repeated in nearly the same number in each successive year. The moral man is subject to laws as fixed as the physical man. Some years ago, Quetelet showed from the comparison of the annual number of deaths in Paris, and the annual number of crimes committed throughout France, that the statistics of human crime are as fixed as the statistics of human mortality; that each age paid as uniform and constant a tribute to the jail as it paid to the tomb; that the numbers of any specific crime in successive years was, like the numbers of deaths from any specific disease in successive years, always nearly the same; ay, that the very instruments by which the same crime (as murder) was perpetrated in different years, were always in nearly the same proportion.[1]

[1] "If all human actions could be registered," says Quetelet, "it might be supposed that their numbers would vary from year to year as widely as human

THIRD PROPOSITION.—*The absolute number of those that recover, should, cæteris paribus, be as fixed as the number of those that die from individual diseases in a given time, in a given population.*—The preceding table (No. II.) shows how many died of the several affections included under it, during a succession of years. If our statistics were more specific and detailed, we ought to be able to tell also how many recovered each year from attacks of each of these affections, as well as how many died from them; and if we could thus count the number of recoveries as well as the number of deaths, by striking the proportion between them, we would obtain the average mortality of each disease. The deaths, for instance, from croup, amount on an average to 4325 each year. But if at the same time we knew the total average number of cases of croup that occurred every year (say, for the sake of illustration, that they amounted to 13,000 in all), then the mortality of the disease would amount to nearly 1 in 3; or out of every three patients attacked with croup, two would recover and one die.

The Registrar's reports, however, do not furnish us with returns of the recoveries as well as of the deaths in any special disease or complication, with one exception. Under the division of births he gives the number of children born each year, and under the term childbirth, in the division of deaths he gives the number of mothers who perished each year under parturition or its consequences. Being thus furnished not only with the annual total number of deaths that took place from parturition, but also with the annual total number of cases

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caprice. But this is not what we in reality observe, at least for that class of actions of which we have succeeded in obtaining a registry. I shall quote but a single example; but it merits the attention of all philosophic minds. In every thing which relates to crimes, the same numbers are reproduced so constantly that it becomes impossible to misapprehend it—even in respect to those crimes which seem perfectly beyond human foresight, such as murders committed in general at the close of quarrels, arising without a motive, and under other circumstances to all appearance fortuitous or accidental. Nevertheless, experience proves that murders are committed annually, not only pretty nearly to the same extent, but even that the instruments employed are in the same proportions."—*Treatise on Man*, p. 6. The following table, abridged from Quetelet, may enforce still more the truth of his observations.

Table of the Annual Total Number of Murders, and Instruments of Murder, in France, collected from the Reports of Criminal Justice, from 1826 *to* 1831.

Modes of Murder.	In 1826.	In 1827.	In 1828.	In 1829.	In 1830.	In 1831.
Total Number of Murders, . .	241	234	227	231	205	266
By Gun and Pistol, ...	56	64	60	61	57	88
By Knife, ...	39	40	34	46	44	34
By Stones, ...	20	20	21	21	11	9
By Kicks and Blows, ...	28	12	21	23	17	26
By Cudgel, Cane, &c., . . .	23	28	31	24	12	21
By Stabs, Cuts, and Bruises, .	35	40	42	45	46	49
By other means, ...	40	30	18	11	20	39

The difference in 1830 and 1831 from the preceding four years were no doubt owing, in a great degree, to the Revolution of 1830, and its immediate effects.

of parturition that occurred, we have the data in this instance for calculating the proportion of recoveries to the proportion of deaths in childbirth. And the following table presents the results for the only four years of which the full data have been yet published.

No. III.—*Table of Proportion of Deaths in Childbed in England and Wales, from* 1839 *to* 1842.

Years.	No. of Children born.	No. of Mothers dying in childbed.	Proportion of Maternal deaths in childbed.
1839	492,574	2915	1 in 169
1840	502,303	2989	1 in 168
1841	512,158	3007	1 in 170
1842	517,739	2687	1 in 192 [1]

Fourth Proposition.—*Statistics enable us to prove that the general mortality, the mortality in particular departments of practice, and the mortality from individual diseases, are capable of being altered by altering the attendant circumstances.*—Under the three preceding propositions, in showing the attendant results to remain from year to year the same, I have supposed the attendant circumstances to remain also the same. But if we change the conditions in which the community or the individuals composing it are placed, we change also the results. The great power which *art* possesses, is the power of altering these attendant conditions and circumstances by improved hygeienic measures, improved medical practice, improved modes of operating, &c. And the effects of this alteration might be interminably questioned and doubted, provided we had not the power of proving it by simple statistical evidence. I shall adduce some examples.

Improve in regard to salubrity, &c., the circumstances in which a community is living, and you increase the value of life in that community, or diminish its mortality. Thus, in 1786, the yearly rate of mortality for the whole of England and Wales was 1 in 42; or, in other words, 1 in every 42 of the inhabitants died annually. In 1801, this mortality was found to be 1 in 47. In 1831, it had diminished to 1 in 58; showing a difference of 38 per cent. in the short period of half a century.[2]

Again, if we wished to prove that our practice, in relation to any

[1] In relation to the etiology and pathology of puerperal fever—the most common cause of death in childbed—it is not uninteresting to remark, that in 1842, when the deaths following parturition were so few, the number of deaths from erysipelas was also much diminished in number. This fact may, I think, be adduced as a reason additional to the many lately urged in proof of an identity in causation and character between erysipelas and puerperal fever.

[2] Dublin Review, vol. ii. p. 97.

particular department of the profession, was more successful now than formerly, or more successful under one mode of treatment than under another, our only certain form of evidence consists in a similar appeal to statistics. Mere assertions and opinions avail little in such a question. Figures and statistics can alone properly decide and determine it. And here, as elsewhere, they offer a kind of evidence, which is not less remarkable for its intelligibility and simplicity, than for its precision and certainty. I shall again take the case of childbirth as an example. The following table, calculated from the mortality bills of London, and given in a more extended form by Dr Merriman,[1] demonstrates statistically—what could not be satisfactorily proved by any other form of evidence—that the practice of midwifery has become more and more improved in London, and inferentially in the whole kingdom, during the last two hundred years; the ratio of deaths in childbed having gradually become less in number, and that to such a degree, that the proportion of parturient mothers lost, during the last years of the 17th century, were nearly double the number of those lost during the first years of the 19th century:—

No. IV.—*Table of average number of Mothers dying in Childbed in London from* 1660 *to* 1820.

Years.	Proportion of Mothers lost.
For 20 years ending in 1680 . .	1 in every 44 delivered.
For 20 years ending in 1700 . .	1 in " 56 "
For 20 years ending in 1720 . .	1 in " 69 "
For 20 years ending in 1740 . .	1 in " 71 "
For 20 years ending in 1760 . .	1 in " 77 "
For 20 years ending in 1780 . .	1 in " 82 "
For 20 years ending in 1800 . .	1 in " 110 "
For 20 years ending in 1820 . .	1 in " 107 "

Further, supposing we desired to prove, in regard to any one complication or disease, that some particular mode of treatment or of operation was more successful than another, we can resort to no other definite mode of decision than statistics; and no other known mode of investigation could yield the same simple and satisfactory results. In illustration, let me adduce an instance from obstetric surgery.—The normal conjugate diameter of the brim of the pelvis is four inches. Sometimes, however, it is morbidly contracted. When, as occasionally happens, this bony canal is so much diminished in size that its opposite walls are not more than from two and a half to three inches distant from each other, the mutilation and destruction of the child's head by craniotomy was formerly supposed, by British accoucheurs, to be the only proper, or indeed possible, mode of delivery. Latterly, after great and strenuous opposition, a revolution

[1] Synopsis of Difficult Parturition, p. 343.

in practice has taken place in such cases; and, at the present day, the artificial induction of premature labour at the seventh month is the established rule of treatment. The child's life has thus certainly been saved, in many instances, from otherwise inevitable destruction. But is the life of the mother not placed, as has been sometimes alleged, in greater jeopardy by it? Statistical evidence answers the question by showing, that while craniotomy is fatal to the mother in about one in every five cases, the induction of premature labour is not fatal to her life in more than about one in every fifty cases. Nor would it be possible to place such a question beyond the possibility of doubt except by statistics, built upon a proper and sufficient basis of data. Mere impressions and arguments would not solve the problem. Dr Osborne declared, for instance, that craniotomy was rarely fatal; and Madame Lachapelle described the induction of premature labour as rarely safe. Statistics have amply belied both opinions.

Fifth Proposition.—*Statistics offer a test by which the impressions of unrecorded and limited experience are corrected; and they furnish a mode of investigation capable of resolving many existing practical problems in surgery.*—It is only since statistics began to be applied to surgical investigations, that surgeons themselves seem to have become aware of the excessive rate of mortality accompanying most of their capital operations. As long as the mere impressions of cases upon the memory was depended upon, and the individual cases or facts themselves not noted or counted, the most erroneous opinions prevailed regarding the rate of mortality following upon surgical practice. All conclusions drawn from the memory are, observes Malgaigne, "horribly fallacious (*horriblement infidèles*), and it is," he adds, "to their employment that we owe the astonishing delusions almost generally professed regarding the real danger or fatality of amputations." [1]

In his System of Surgery, Mr Benjamin Bell, one of the most esteemed surgical practitioners and writers of his day, in some general remarks upon amputation, observes, "In the present improved state of the operation, I do not imagine that one death will happen in twenty cases, even including the general run of hospital practice: and in private practice, where due attention can be more certainly bestowed upon the various circumstances of the operation, the proportion of deaths will be much less." [2] But in 1844, Mr Inman collected the statistics of 3586 cases of "amputations generally, including secondary, primary, for accident or disease;" most from hospital, but some from private practice, and he found that out of these 3586 cases, there died 1146, or 1 in every $3\frac{1}{10}$. [3] In a very

[1] Archives Générales de Medicine for April 1842, p. 391.
[2] A System of Surgery, by Benjamin Bell. Vol. vii. p. 254 of 7th edition.
[3] Lancet for 5th October 1844, p. 39.

valuable communication, Mr Fenwick has published a collated table of 4937 amputations. Out of these 4937 cases 1565 died, or the operation was fatal in the proportion of 1 in every $3\frac{1}{15}$ submitted to it.[1] The late statistical investigations of Phillips, Lawrie, Malgaigne, Gendrin, &c., have all fully borne out the same view with regard to the great mortality attendant upon amputations.

Nor are opinions formed from a single accurately observed case, or a very limited number of data, to be depended on as the ultimate probable measure of the value or fatality of an operation. An impression from an individual case often leads us to form a wrong estimate of the average danger or average safety of an operation, while adequate statistics at once show us the truth. Mr Pott[2] saw amputation at the hip-joint performed in one case, and from that case drew the general deduction, that in this operation the want of success would be uniform. Mr Syme, after performing amputation at the hip-joint in one case, from that case drew the opposite deduction. "I firmly believe[3] (says he), that if the operation be done properly, and above all, quickly, its success will be general, if not uniform." In his excellent "System of Surgery," Professor Fergusson of London, depending upon his impression of cases, but not actually counting them, observes in regard to amputation at the hip-joint, "although no reasonable practitioner would ever attempt it except as a last resource, it is somewhat satisfactory to know, that with all the disadvantages under which it has been performed, particularly in military practice, and notwithstanding the fearful shock that must of necessity attend such an extensive mutilation, the success of the operation has probably been such, that 1 patient out of every 3 on whom it has been performed has been saved."[4] But when we turn from such conclusions to the simple evidence of numerical facts, all of these three opinions are seen to be erroneous; and, in addition, the mind at once obtains a precise and definite idea of the degree of danger attendant on the operation when we simply appeal to statistics and find, as Mr Sands Cox has shown,[5] that out of 84 cases in which amputation at the hip-joint has now been performed, 26 were successful, and 58 unsuccessful; or, out of every 10 operated upon, 7 died and 3 recovered.

Now the certainty and correctness of the knowledge which we

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[1] Monthly Journal of Medical Science for October 1847, p. 238.

[2] Chirurgical Works. Vol. iii. p. 217, 218.

[3] Edinburgh Medical and Surgical Journal. Vol. xxi. p. 27. "Successful case of Amputation at the Hip-joint." The patient died "at the commencement of the eighth week from the operation."

[4] Practical Surgery. Page 362. The rate of mortality which Mr Fergusson here attributes to amputation at the hip-joint, is in fact the rate of mortality belonging to all the *minor* amputations of the limb, taken as a whole, as has been shown in the preceding paragraph. The actual fatality of amputation of the hip-joint is above 2 in 3, instead of being 1 in 3.

[5] British and Foreign Medical Review. July 1846, p. 112.

obtain in this and similar instances from merely and simply *counting* up a hundred accurately recorded cases, is infinitely superior to a hundred separate opinions and arguments upon the matter. A hundred writers upon such a subject as the probable degree of fatality accompanying amputation at the hip-joint, would no doubt give us every variety and confliction of opinion on the subject. A hundred cases of it correctly noted and counted, would give us a result not admitting of any variety or confliction of opinion, except it were objected that the numbers on which the calculation was founded were too small for a perfect conclusion; and this objection can always be met by collating additional data in order to extend the basis of our calculation, and thus remove and free it from this chance of possible error.

Upon my own mind, the strongest conviction is impressed, that the numerical or statistical method of inquiry is yet erelong destined to advance and promote surgical science, by revolutionizing some departments of surgery, by rectifying a number of its existing errors, by clearing away many of its doubts and difficulties, and by settling and determining for it definitely, various of those practical questions upon which the opinions of the best operators are constantly and ever changing. In the introductory remarks to his last work on surgery,[1] the late Sir Charles Bell remarked, "Men's opinions go to extremes; they vibrate like the pendulum." But the application of statistics to surgery will betimes impart greater precision, and accuracy, and stability to its opinions; for it forms, I believe, the simple, and, at the same time, the only possible means of deciding numerous doubtful and disputed questions in the practice of it. It has, for example, been much and long debated whether the circular or the flap method of amputation is the safer and the better mode of operating. The most opposite sentiments are still expressed, and the most opposite practices still prevail in reference to this point. Some surgeons and some surgical schools earnestly maintain the preferableness of the one method; and others as strongly uphold the greater safety and greater propriety of the opposite plan. Even the same mind, with every anxiety to arrive at nothing but the truth, may repeatedly change, and at different times hold different opinions upon the matter.[2] But the question at issue between the flap and circular methods of amputation is principally this;—which

[1] Institutes of Surgery, p. 22.

[2] In the first observations which Mr Syme published on amputation, (Edinburgh Medical and Surgical Journal, Vol. xxi. p. 31,) he strongly maintained "that the circular mode of amputation is in every point of view bad;" and, writing in 1842, he still held that "amputation of the thigh ought always to be performed by making flaps."—(Principles of Surgery, p. 156). In 1845, Mr Syme believing, from statistical evidence, "that there is something radically wrong in the principle" of amputation of the thigh, both by the flap and circular methods, proposed in their stead amputation of the knee; and thus dividing the thigh-bone through its condyles, instead of through its shaft.—(See Monthly Journal of Medical Science for May 1845, p. 337). In the same

operation least endangers the patient's life? Now this question is one which could be satisfactorily settled by statistical investigation, and no doubt will ultimately be so. Probably one or two thousand amputations of the limbs are performed every year by the hospital surgeons of the United Kingdom. If our object were to ascertain whether amputation of the thigh by the flap or circular methods were the safer as regarded the *life* of the patient, and our hospital surgeons were only to note carefully and collect the results of this operation for a year or two, so that we should have the statistical returns of both operations and their results upon a sufficient number of unselected cases, we would thus become furnished with data, the mere counting up of which would show us (infinitely better than any argument) whether the two modes of amputation differed at all in ther relative degree of fatality; and if they did differ, which was the most dangerous of the two; and what was the degree of the comparative excess of danger of the one over the other. By the same form of statistical inquiry upon the same or other cases, and by analysing or decomposing each separate case into as many parts as it contained distinct objects, we could further ascertain and determine all the various minor points, such as—which operation required the shortest period of convalescence—which ultimately secured the best form of stump, &c. &c.

And here I would beg to add one observation relative to the probable future importance and bearing of statistics upon surgery. It will be found that, in most of the past literature of their profession, surgeons have almost invariably contented themselves with recording their own deductions from their own cases, without recording the cases themselves. They have left us their inferences, but have not left us the grounds and bases of these inferences. They have generally given us, not their individual cases or individual facts, but the opinions which they themselves thought fit to draw from these facts.[1] The result has been, that, in numerous instances, inferences of the most erroneous and contradictory kind have been drawn, in consequence merely of the elementary facts observed and generalized upon, being far too few for the establishment of a correct deduction. I have already offered an instance of this in the two opposite opinions expressed by Mr Pott and Mr Syme regarding amputation at the hip-joint, from single cases observed by each, as contrasted with the actual and ascertained degree of danger connected with that operation. A sufficient series of individual facts, collected from the practices of several different surgeons, may thus point out a deduction

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Journal for November 1846, (p. 225,) he does "not persist in advocating amputation at the knee," but avows himself now satisfied that the old circular method of amputation may be "employed at the lower third of the thigh safely and advantageously," "and should be preferred to the flap operation at a higher part of the limb, when the circumstances afford room for choice."

[1] In the past records of midwifery, on the other hand, we have all their important *individual* facts and cases left on record for us, in the works of Mauriceau, Portal, Giffard, Smellie, &c. &c.

quite at variance with the so-called experience and opinion of the individual authors themselves. Few surgeons allow that strangulated hernia, when properly operated upon, is very fatal in its results. "The operation," says Mr Pott, "if applied to in time, very seldom fails; so seldom, that I believe I might venture to say, *not* 1 in 50 dies of it if timely and judiciously executed."[1] But, out of 77 cases recorded in Sir Astley Cooper's work on hernia, 36 died; out of 183 operations for this disease, collected by Malgaigne, 114 proved fatal; out of 545 collected by Dr Inman, 260 died; or 1 in every 2 perished instead of 1 in 50. Again, amputation of the thigh is fatal in nearly the same proportion; or about 1 in every 2 or 3 who are subjected to it dies. Out of 987 cases of this operation collected by Mr Phillips,[2] 435 of the patients, or 1 in every $2\frac{2}{10}$ perished. Yet, speaking of the degree of danger accompanying amputation of the thigh, as apparently inferred from his own observations upon the point, Mr Ormerod, in a late work on surgery, observes that, "considering the severity of the operation, and extent of the injury done to an individual by the removal of a limb above the knee, the success of the amputation there is very great; the patients are often placed under bad circumstances for operation, and their health very much reduced, yet death from failure, after the removal of a limb for disease, is *very rare* indeed."[3] I repeat, that if, instead of dealing in vague and valueless generalizations and opinions of this stamp, surgical authors would only carefully note and record all their individual operations and results, with a view that betimes a sufficient collection of data might be thus gradually gathered together, in order to settle particular questions of surgical science or practice, then their collated facts would, in determining such questions, be indescribably more valuable than their individual opinions. For the facts and testimony of surgery would thus become *cumulative*, and increasingly conclusive upon any points on the investigation of which it was deemed proper to direct the power of its evidence, instead of being lost and frittered away, as at present, on the formation of a host of isolated opinions, which are too often not less perplexing from their contrariety than from the confidence and dogmatism with which they are severally advanced. It is interesting to reflect how much, in all of these respects, might soon be accomplished by proper and systematical annual reports from the great public surgical hospitals throughout the country; and at the same time it is distressing to consider what masses of valuable information are yearly lost from the mere want of such reports.

SIXTH PROPOSITION.—*Statistical Evidence alone enables us to ascertain correctly the effects of various minor conditions upon the Fatality of Operations,—such as the influence of the age, sex, &c., of the patient; the special success of different operators, &c.* The

[1] Chirurgical Works, Vol. ii. p. 180. [2] Medical Gazette for 1844. P. 805.
[3] Clinical Collections and Observations in Surgery, p. 135. London: 1846.

results of surgical operations are, like the results of diseases, varied by age, sex, constitution, idiosyncrasy, &c. On the influence of these, and other minor points, some surgeons may have been led to form and express opinions more or less correct; but it is only by employing the numerical or statistical method of examination, that a perfect degree of accuracy of judgment can be possibly attained on such matters. Without statistics, all opinions on these points would have remained doubtful and undetermined; by statistics, their influence can be at once discovered and measured, and that, too, by a kind of evidence which is at once simple and convincing. I shall adduce one or two points as an example of the whole.

Let us consider the influence of *age* upon the results of the operation of lithotomy. Various late authors have published the ages of their patients, and reports, which might be reduced to show the influence of age upon this operation. I shall content myself with tabulating, for this purpose, the earliest of the kind ever published; viz., those of Cheselden. He has left records of the ages and results of lithotomy, in 213 cases operated upon by him at St Thomas' Hospital.[1] Out of these, only 20 patients died, or the small number of 1 in $10\frac{13}{20}$. But I shall throw all the 213 cases into a tabular form; and it will be at once seen, from this view of Cheselden's recorded data, that the danger of lithotomy increases in a ratio progressive with the age of the patient.

No. V.—*Table showing the influence of the Age of the Patient upon the Mortality of Lithotomy.*

Ages of the Patients.	Number of Cases.	Number of Deaths.	Ratio of Mortality.	Per Centage of Deaths.
Under 10 years, .	105	3	1 in 35	3 in 100
From 11 to 20 years,	62	4	1 in $15\frac{1}{2}$	6 in 100
From 21 to 40 years,	22	5	1 in $4\frac{2}{5}$	22 in 100
From 41 to 80 years,	24	8	1 in 3	33 in 100

Let us take another illustration from lithotomy of the capability of statistics, proving one more of these minor points, such as are alluded to in the general proposition. Without statistics, it would be difficult or impossible to demonstrate the influence of the mere *size* of the stone extracted upon the results of the operation of its extraction. But by statistics it can be readily proved that the mor-

[1] Cheselden's Anatomy, p. 332.

From the admirable researches of Mr Edmonds and Mr Farre, we know that the mortality of disease *in general*, and the mortality of *individual* diseases (as small-pox, &c. &c.), increases from puberty upwards, in a regular geometrical progression, and that the rate of increase is about 3 per cent. every year, or more nearly 34 per cent. every 10 years. I believe, that an adequate collection of data will very probably show that this same "constant" of mortality regulates the degree of liability to death in lithotomy, amputation, and other surgical operations.

tality of lithotomy rises higher and higher in proportion as the stone increases in size; and hence, in all probability, in proportion as the operation increases in severity and difficulty. The following table, calculated from the Norwich data furnished by Mr Crosse, in his valuable work,[1] affords the required numerical evidence for this generalization.

No. VI.—*Table of the mortality of Lithotomy, calculated according to the different Weights of the Stone extracted.*

Weight of Stone.	Number of Cases.	Number of Deaths.	Ratio of Mortality.	Per centage of Mortality.
2 oz. and under	648	65	1 in $9\frac{63}{65}$	10 in 100
From 2 to 4 oz.	46	23	1 in 2	50 in 100
From 4 to 7 oz.	9	5	1 in $1\frac{4}{5}$	55 in 100

Statistics in surgery have been objected to on the ground, that in combining cases, in order to arrive at a general result, we do not take cognizance of the superiority of the practices of individual operators. "We find (argues Dr Bennett) operations by different surgeons, and various experiences, all mingled together to produce one sum total.[2] The most skilful metropolitan surgeon is put on a par with the country practitioner, and the experience of long practice is of no more value than that of the tyro. It is well known that, even in one person's practice, he operates differently at different periods of time. Mr Syme had well illustrated this with regard to lithotomy, and told us that, since his alteration of the method of its performance, his success had been much greater than formerly. Yet, according to the reasoning of Dr Simpson, all the operations must be added together, and those performed during the inexperience of youth and the senility of advanced life, must, as with Mr Martineau, be put on the same level with the cases that form the boast of mature age, and the most perfect powers of mind and body." [3]

Now, in this as in other points, I believe that the statistical method of inquiry forms the means, and the *only* means, of enabling us to prove the very items which it is alleged that statistics lose sight of and conceal. Take, for instance, the identical examples adduced. The present *general* average mortality of lithotomy, as performed by *all* operators on subjects of all ages, is, according to Dr Willis, about 1 in 8.[4] Out of 5900 cases collected by Mr Inman, 765 patients

[1] Treatise on Urinary Calculus, p. 162.

[2] Of course this is necessary when we wish to ascertain the *general* average success of an operation in the hands of *all* surgeons, and not its *special* average success in the hands of any *individual* operator or operators.

[3] Monthly Journal of Medical Science, October 1847. P. 307.

[4] "The average mortality from lithotomy, on all hands, appears at present to be about one in eight."—Dr Willis' Urinary Diseases. 1838. P. 347.

died, or 1 in $7\frac{3}{4}$. Out of 14 cases operated upon by Mr Syme, and recorded in his surgical reports in the Edinburgh Medical and Surgical Journal (vol. xxxiii. to vol. xxxix.), 5 died, or 1 in $2\frac{4}{5}$. Since adopting his present plan of lithotomy, however, he had performed 17 operations in the hospital, of which 2 only have proved fatal, or 1 in $8\frac{1}{2}$. Now, this difference could not be educed or stated with accuracy in any other way than by figures, or by the statistical method; for by it alone can we determine the *special* averages of different operators, or of the same operator at different times. But "take care (observed Sir Astley Cooper) how you draw any deduction from particular cases. I and many others have for a length of time met with extraordinary success in operating for the stone, when 4 or 5 unsuccessful cases in succession have come, which have generally brought down the result to the amount I mentioned, viz., that 2 in 15 die."[1] Mr Martineau's practice afforded a curious illustration of the necessity of this caution.

In the 11th volume of the "Medico-Chirurgical Transactions of London," Mr Martineau published an account of 74 cases, in which he had performed the operation of lithotomy in the Norwich Hospital from the year 1804 to 1840.[2] Only 2 of these 74 died, or 1 in 37. We learn further, however, from a paper of Dr Yelloly,[3] that Mr Martineau operated in the same hospital on 73 additional cases, (147 in all). Out of these 73 additional cases, 15 died, or 1 in $4\frac{9}{10}$. And I repeat, that it is statistics only which could properly and fully prove to us this great *special* difference in the success of Mr Martineau's practice at different periods. At the same time, however, the same case proves to us further, that if we wished to obtain not this *special* average of practice at a *selected* time, but the *general* average of all his practice at all times, it would amount to nearly the general average of most other operators. For out of his whole 147 cases, 17 died, or 1 in 8, which we have seen to be nearly the common degree of success in lithotomy, according to the investigations of Drs Willis and Inman. The special average success of some operators has been greater than this. We have already seen that Cheselden, out of 216 recorded hospital cases, only lost 20, or 1 in $10\frac{1}{3}$. The special average success of other operators has been less. Out of 356 Parisian cases collected by Dupuytren, 61 died, or 1 in 6. Such differences, I repeat, could never be proved and substantiated, unless by statistics.

SEVENTH PROPOSITION.—*Statistics afford us in general the only true and ultimate "measure of value" of any proposed alternative oper-*

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[1] Lectures on Surgery. P. 321.

[2] He records 84 cases in all, with 2 deaths; but 10 of these 84 cases had occurred in private practice.

[3] Philosophical Transactions for 1829, p. 63. "The whole number of Dr Rigby's operations [in the Norwich Hospital] was 106, with 15 deaths; and of Mr Martineau's 147, with 17 deaths."

ation, or of any new practice in surgery. Every well-informed writer has naturally and almost instinctively recourse to this form of proof, when originating a new, or reviving a neglected operation. For example, in his able treatise on "Excision of Diseased Joints," Mr Syme, in recalling the attention of surgeons to the operation of Park and Moreau, and in showing the advantages and safety of excision of the elbow joint, as compared with the alternative operation of amputation of the arm, most properly uses the following statistical argument: "I have," he remarks, "cut out 14 elbow joints, and the operation has been performed in Edinburgh three times by other practitioners; of all these 17 cases, only 2 have terminated fatally; and in one of them the patient would, I believe, have died from any operation whatever, while, in the other, the disease was found so extensive as to render the excision almost impracticable. I believe the result of 17 amputations in similarly unfavourable constitutions would not be so satisfactory."[1]

Anatomical and pathological or other considerations may suggest to us the propriety or impropriety of any newly proposed condition or mode of operating; but an appeal to statistics is the only means of ultimately and definitely deciding upon its merits or demerits. Every newly-proposed practice in surgery thus offers, as it were, a new problem for statistical solution. For instance, surgeons were long afraid to place a ligature upon the carotid artery, fearing the difficulties of the operation, and the probabilities of danger to, and derangements in, the cerebral circulation. Statistics, however, show that this reasoning was so far unsound. Ligature of the carotid has now been performed above two hundred times.[2] Out of that number it has proved fatal in the proportion of about 1 in every 4 patients operated on; and consequently it has become an established operation in surgery,—many capital operations being more mortal than this.

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[1] Treatise on the Excision of Diseased Joints, p. 26.

[2] I have collected the following table from an elaborate essay of Mr Norris, on Ligature of the Carotid, &c., published in the 27th Number of the American Journal of the Medical Sciences.

Table of the Statistical Results of 203 *Cases of Ligature of the Carotid Artery.*

Cause for Operation.	No. of Cases.	No. of Deaths.
Aneurisms	38	16
Wounds, &c.	30	15
Extirpation of Tumours	18	6
Cerebral Affections	6	0
Erectile and other Tumours, &c.	42	13
Brasdor's Operation	15	4
Total.....	203	54

After seeing the comparative safety with which ligature of the carotid and other large arteries was performed, surgeons reasoning on these results, believed it would be justifiable to tie the arteria innominata. The results, however, have belied the *à priori* reasoning. Ligature of the arteria innominata has now been performed, according to Mr Norris, in eleven cases. All the eleven patients died.[1] In three other cases, occurring in the practice of Post, Key, and Porter, the operation was commenced, but not completed. Two of these three patients died. In the third (Mr Porter's) the idea of tying the artery was abandoned after it was exposed; the wound was closed up, and the patient recovered. And, doubtlessly, these statistical results will, ere long, compel surgeons to acknowledge this operation to be one which it is unjustifiable in them to practise.

EIGHTH PROPOSITION.—*The objections of late years offered against the application of statistics to practical inquiries in surgery and medicine, seem altogether founded upon a misconception of the objects and principles of statistical investigation.*—The application of the statistical or numerical method of inquiry to the solution and determination of questions in medical and surgical science, is of comparatively late date.[2] Like most other innovations, its introduction has been more or less strongly opposed;[3] and the principal objections which have from time to time been urged against the employment of it, have been the following:—

1*st Objection:*—*The numerical or statistical method consists of a calculation of probabilities.* There is no doubt whatever of the truth of this allegation. But if it formed a valid objection against the application of statistics to medicine and surgery, it would form equally a valid objection against almost all other modes by which the human mind struggles to acquire increased knowledge, either in medicine or in any other department whatever of science and art. For, as the great French mathematician Laplace observes,—and I could not quote a greater and higher authority on such a point,—"To speak strictly, almost all our knowledge is but probable; and among the small number of things which we can know with certitude, in the mathematical sciences themselves, the *means* to arrive at truth are founded upon probabilities; so that the *entire*

[1] The operations in these eleven cases were performed by Mott, Graefe, Hall, Dupuytren, Norman, Bland, Lizars, Hutin, Arendt, Liston, and Kuhl.

[2] See some historical notes regarding it in a paper by Mr Marshall, in the Edinburgh Medical and Surgical Journal, No. 116.

[3] Our semi-civilized brethren of China, with their fixed hatred of all improvements and innovations, seem to have a particular dislike to statistics, and upon grounds amusingly similar to those of some members of the medical profession in Europe. "Moreover (says Mr Fortune), they [the Chinese] cannot appreciate statistical inquiries; they always fancy we have some secret motive for making them; or that the subject cannot be of the slightest importance either to ourselves or others."—*Three Years' Wanderings in China*, p. 3.

system of human knowledge is subjected to the theory of probabilities." [1]

2*d Objection:—The numerical method calculates together as similar, facts which are not sufficiently similar to be a basis of calculation of probabilities.* M. Double and others who have propounded this objection, have affirmed, that no two cases in medicine or surgery are entirely or exactly the same, and hence that they cannot be counted together as the same. But if this strange and illogical averment were true to the extent to which its supporters maintain, and if every single case to which the physician was called, and every single operation which the surgeon performed, were an individuality and unity so dissimilar from all other previous cases of disease or operation which he had witnessed, as to be incapable of being grouped or classed in any way with them,—then we could not possibly have any general facts, principles, or rules to guide us in the practical exercise of our profession. And, if medicine and surgery had no such general laws or principles, there would be necessarily an end to their existence, either as sciences or arts. Grant, however, that there are specific diseased states in medicine, and specific operations in surgery, with some general facts or rules applicable and peculiar to each disease, and each operation; then, it is further evident, that each of these general facts must have been originally founded upon a basis or deduction of particular facts,—that the fundamental particular facts must be always *more* or *fewer*—consequently capable of being counted,—and, consequently, coming within the range of the numerical method of inquiry.[2]

But it has been further objected, allowing that individual cases of disease may be arranged into groups or species, still the individual cases composing these groups are often so inaccurately *observed* as not to form a sufficiently true basis for statistical comparison and inquiry. This objection, however, applies to all other modes of medical investigation as well as to the numerical. There is precisely this very same difficulty to overcome, in whatever way or by whatever method we attempt to study and generalize upon diseases. There is

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[1] Essai Philosophique sur les Probabilités, p. 1, &c.

[2] If the reasoning of M. Double were admitted, "it would," as Gavarret properly observes, "altogether strike down medicine from the position which it ought to occupy in the temple of human knowledge. What language," he adds, "can a physician address to his pupils, who will not see, any where, but individualities? On what ground can he recommend them such or such treatment for their patients, since they ought never to meet, in their practice, any thing comparable with what their master has seen? According to this inadmissible hypothesis, medical experience would be a word without meaning; the student who has never yet seen a patient, would necessarily know as much as the most perfect physician. For if the career of the latter, and his predecessors, is consumed in the sterile observation of a succession of therapeutic individualities, the healing art cannot but be composed of a series of isolated attempts, without a common tie, and from which it would be impossible to draw any general conclusion, or any precept for the future."—*Principes Généreaux de Statistique*, p. 42.

this difference, however—the statistical method compels, if possible, and exacts *more* care, and caution, and correctness in our study, and in our records of cases than other plan of generalizing; and, certainly, *this* forms an argument in favour of the adoption of statistics, rather than an argument in favour of the rejection of them. For it is an acknowledged truth, in medical as in all the other sciences, that the greatest attainable degree of accuracy in our fundamental or elementary facts is necessary, that we may reach the greatest attainable degree of accuracy, and consequently of utility, in the general practical conclusions or laws which we venture to deduce from these facts. From time to time we are obliged, in *every* known form of medical reasoning and generalization, to revise our fundamental facts, and change or modify our conclusions as our knowledge of pathology, diagnosis, &c., increases. The same holds true of the numerical method. And at present, the principal obstacle against applying statistics, more fully than has been done, to some departments of the physician's study, confessedly consists in our occasional inability to make a perfect and undoubted diagnosis of some *internal* diseases, and hence in the liability of our comparing and calculating together cases that are not specifically similar. In statistics, however, as applied to surgery and surgical questions, the same obstacle does not meet us, at least in the same degree. Supposing we wish, for instance, to calculate what proportion of persons dies under particular amputations,—we only require perfect accuracy on three facts, each and all of which could certainly be noted, and, by a little care, noted with perfect accuracy, and without much, if indeed any, chance of error. For they amount to these points in each case, viz.: 1. Was amputation performed? 2. In what part of the limb was it performed? and 3. After its performance, Did the patient live, or did he die? In this and many other points of surgery, to which the numerical method of inquiry is capable of being directed, all the necessary elementary matter could assuredly, with any common attention and accuracy, be readily collected without much probability or possibility of error. Of course, it is unnecessary to add that in this, as in all other modes of philosophizing, our observations and deductions must be pursued with stern and strict honesty, and with a view to the attainment of truth, and truth only; and that, for this purpose, *all* the pertaining individual facts or cases must be always given, and always counted. There must be no omission; no concealment; no selection of any kind.

3*d Objection:—The statistical method of inquiry is different from and opposed to the inductive method.*—No opinion could be more erroneous. The numerical method is assuredly not opposed to that strict observation of individual facts, and that strict generalization of them, which constitute the double basis and essence of the inductive method; but the very reverse. 1. It demands in the same way the strict observation of individual facts; but it demands that the observation and record of them be made, if possible, with still greater care and accuracy than heretofore. 2. It educes in the same way the

general principles or laws of practice from the comparison and analysis of these observed facts; but it enforces more rigorous accuracy than heretofore in the deduction of these principles or laws, in proportion as figures are more certain than memory, and actual enumerations more certain than general impressions. Its required mode of observation and mode of generalization are the same as in the common inductive method, only more rigid and hence more rigorously correct. Its object is not to supplant but to supplement our former methods of inquiry,—not to make them useless, but to make them more useful by making them more accurate. It is an instrument which enables us to draw our deductions, not only with greater simplicity, but also with greater truth and precision. "No man (says Bacon), be he ever so cunning or practised, can make a straight line or perfect circle by steadiness of hand, but this may be easily done by help of a rule or compass."[1] And those who maintain that the numerical method is different from and opposed to experience and induction, might as rationally argue, that when we try to ascertain the *general fact* of the rapidity of a patient's pulse at a given time, we employ two different methods when we attempt to attain the desired information *without*, and *with* the aid of a stop-watch. We here practise two methods that are not different or opposed to each other. They are logically the same in all respects. But the one method is relatively more accurate, precise, and determinate than the other. And in this as in other applications of the numerical or statistical method, the language we employ becomes at the same time simple and decisive. For if we say, for instance, that the beats amount to 120 a minute, our language is far more clear and simple than if we mentioned that the pulse was "*quick*," or "*rapid*," or "*frequent*," &c. We state a definite and intelligible fact, instead of using some comparatively indefinite and uncertain term, which the very temperament of every speaker and hearer may interpret differently. The great aim and objeet of the statistical or numerical method of inquiry, as applied to surgery and medicine, is the determination of their general facts, or general laws, with the utmost attainable degree of accuracy; but, in truth, figures are not only the strictest and most correct way of *educing* their general facts from the analysis of the *existing* data,—they form also the shortest and most correct way of *stating* or *expressing* these facts after they are educed.

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[1] See Bacon's Advancement of Learning, in Montague's edition, vol. ii. p. 182.